

Reviewing the Riddle of Relativity

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I describe my collaboration with Professor Herbert Dingle in his campaign against Einstein's special theory of relativity, and my subsequent attempts to draw attention to the inadequate response by scientists to his criticisms. Our active collaboration started with the publication of Dingle's book *Science at the Crossroads* in 1972 and continued until his death in 1978. This paper celebrates the 40th anniversary of that book, and points out that the dogmatic adherence of scientists to the special theory has continued to make it difficult for honest and informed criticism to be heard. Two arguments against the special theory are presented, both of which a very distinguished mathematician tried to refute but failed.

1. Introduction

This paper describes some of my activities over a forty-year period of critical study of Einstein's special theory of relativity, which started with my collaboration with Herbert Dingle, a very eminent critic of the special theory. These activities are described more fully in my book **A Scientific Adventure: Reflections on the Riddle of Relativity** [1]. Since my active collaboration with Herbert Dingle started after the publication of his book **Science at the Crossroads** [2] in 1972, I am taking this opportunity to celebrate the 40th anniversary of the publication of his book.

Although I had been skeptical about the orthodox solution of the twin paradox for many years since 1948, I did not have enough knowledge about it at that time to make a rational decision about it. I was aware many years ago that there was a debate going on in the scientific literature, and that the two main participants were Professor W.H. McCrea, who believed that the special theory entailed asymmetrical ageing, and Professor Herbert Dingle, who believed that it did not. Since both these professors had published books on relativity, there seemed to be no point in my trying to settle the matter by myself.

2. My Collaboration with Herbert Dingle

Eventually, in 1971, when I was on sabbatical leave in England, I became aware that a debate was still going on about special relativity, and I decided that I would never have a better opportunity again to try to understand the problem. Since I believed that Dingle had been right in maintaining that the theory did not entail asymmetrical ageing, I wrote to him to express some of my thoughts on the subject.

In his reply, Professor Dingle made it clear that he was not interested in discussing whether asymmetrical ageing occurred or not, since he had gone beyond that to argue that the special theory was internally inconsistent. He described his reasoning in the following words:

"A paradox arises when two apparently sound but contradictory conclusions, X and Y, result from the same premises P. It can be resolved only by (1) disproving X; (2) disproving Y; or (3) finding a contradiction inherent in P. All additional proofs of X or Y do nothing without a disproof of Y or X. Years ago I believed P sound and X (symmetrical ageing) sound, and tried to disprove Y. I could not do so to my full

satisfaction, and that led me to discover a contradiction in P... It therefore seems to me unimportant to compare the merits of X and Y."

Although Herbert Dingle was not interested in discussing the twin paradox, he did tell me about Marder's book on the subject [3], which had then been recently published. I quickly acquired a copy of the book, found it a very useful historical study of the controversy surrounding the paradox, and later used it as a basis for discussion of various aspects of the results of asymmetrical ageing in Chapter 3 of my book. However, I went on to agree with Dingle that there appeared to be an inconsistency in the theory.

In his arguments about the special relativity problem, Dingle chose as his canonical text Einstein's original paper on the subject, published in *Annalen der Physik* in 1905 and later in an English translation in 1923 [4], and based his book **Science at the Crossroads** on that choice. In a similar way, I stated in my own book that the central point of my book was the following question: Is there, or is there not, an internal inconsistency in Einstein's special theory as described in his original paper on the subject?

The answer to that question depends on the logical properties of the theory, as expounded in Einstein's original paper; so it can be answered only by examining that paper or a reliable translation of it. The theory is a product of a human mind, not a law of nature, so the answer to the question of its self-consistency does not depend on the physical properties of the rest of the world and cannot be obtained by doing physical experiments. Furthermore, if the question had been asked in 1905, just after the original paper had been published, it could have been answered definitively at that time by examining the paper, so the answer cannot depend on any subsequent events. Yet some of Dingle's opponents used appeals to experimental evidence and to the general relativity theory, which did not appear until about a decade after the special theory, to try to refute his claim of an inconsistency.

After becoming active in studying the problems of the special theory, I tried to publish some papers that were critical of the theory. Although my early efforts were almost all unsuccessful, I became aware of some of the many irrational reasons that were given by editors to reject critical papers, and I described some of the rejections of my papers in the book. After Dingle's death, I was able to arrange for the publication of his last scientific paper

[5] in *Wireless World* in 1980, and after that I published a paper of my own in the same journal [6]. This paper, "Problems in Special Relativity", described many published attempts to refute Dingle's thesis that there is an inconsistency in the special theory; some of these published rebuttals were from reviews of **Science at the Crossroads**, and some were from the published correspondence in *The Listener* in 1971-72 that had first drawn my attention to the continuing controversy about the theory. My article drew attention to many inconsistencies among the many arguments that were used to try to refute Dingle; I pointed out the obvious fact that they could not all be correct, and they might all be wrong.

After Dingle's death I also published part of the story of my collaboration with him, in which, among other subjects, I documented some of his correspondence with various people in an attempt to reopen the discussion of the problems that he had raised in **Science at the Crossroads**. This account, called **The Relativity Question**, is available on the NPA World Science Database [7].

3. My Debate with Professor Good

One of the vulnerable parts of the special theory is Einstein's statement that a clock at the equator would work more slowly than a similar clock at one of the poles, and I used that statement as a basis of a critical argument that I published in *Physics Essays* [8]; this argument later became the starting point of my debate with Professor Good, which I described in Chapter 8 of my book. I now quote that statement as it occurs in the generally accepted English translation of Einstein's paper [4]. The following two statements (without the labels E1 and E2) are direct quotations, and together they constitute a single paragraph of the paper; E2 is the statement about the polar and equatorial clocks:

E1. If we assume that the result proved for a polygonal line is also valid for a continuously curved line, we arrive at this result: If one of two synchronous clocks at A is moved in a closed curve with constant velocity until it returns to A, the journey lasting t seconds, then by the clock which has remained at rest the travelled clock on its arrival at A will be $t v^2 / 2c^2$ seconds slow.

E2. Thence we conclude that a balance-clock at the equator must go more slowly, by a very small amount, than a precisely similar clock situated at one of the poles under otherwise identical conditions.

That statement about the polar and equatorial clocks was mentioned several times in my debate with Professor Jack Good, which is described in Chapter 8 of my book [1], and I used the labels E1 and E2 there also.

The argument in my paper [8] can be summed up very simply. I started by referring to Einstein's statement about the polar and equatorial clocks, and then stepped backwards in his argument to the case of a clock moving around a polygonal path. I considered a clock A at the center of a square, and supposed that another clock B moved around the perimeter of the square at a very high constant speed. According to special relativity clock B would lose a definite amount of time in each complete tour around the square, and by symmetry it would lose a quarter of

that amount while moving along each side. Hence B would work steadily more slowly than A while travelling along any one side. Now suppose that a third clock C moves at uniform speed along an infinite straight-line path in such a way as to travel alongside B while B is travelling along one side of the square. Then C would work at the same rate as B, steadily slower than A. But, by the principle of relativity, we can equally well show that A must work steadily more slowly than C since A and C are in uniform relative motion throughout, so there is an inconsistency.

Throughout most of our published debate, Good agreed that Einstein's statement E2 led to a contradiction with the kinematics of the special theory, but he claimed that Einstein was incorrect in that statement; he called it "Einstein's Slip." At the very end of the debate [9] he conceded that it was not a slip after all and apologized to me, but he did not withdraw his statement that it led to a contradiction. This pointed out a serious inconsistency in his argument. Unfortunately he died before the matter could be further resolved, but I think it is fair to say that he failed to refute my argument.

In my debate with Jack Good I presented a discussion of the properties of an inconsistent theory [10], and I now give a simpler version of the same argument. In order to avoid ambiguity, I shall use the usual symbols of symbolic logic: if we have statements p and q , $p \bullet q$ means p **and** q , $p \vee q$ means p **or** q , and $\sim p$ means **not** p . Suppose that there is a theory **T** having, like special relativity, two postulates, which I call p and q . Now suppose that p and q are inconsistent, that is: they cannot both be true, though they might both be false. I can represent that property by saying $\sim(p \bullet q)$. We represent the properties of this inconsistent theory by writing:

1. p
2. q
3. $\sim(p \bullet q)$

But it follows from the first two postulates that $p \bullet q$. The *inconsistency* led to a *contradiction*, since $p \bullet q$ and $\sim(p \bullet q)$ contradict each other. Now suppose that we wish to derive some arbitrary proposition s from my theory **T**, we can do it using the disjunctive syllogism in the following logical steps:

$$\begin{aligned} & (p \bullet q) \vee s \\ & \sim(p \bullet q) \\ & \therefore s \end{aligned}$$

The proposition s can be any statement that we wish, such as "The earth is flat." I have not proved that the earth is flat; I have shown that that statement can be derived, using valid logical rules, from an inconsistent theory. We could equally easily derive the statement "The earth is not flat." Not surprisingly, a contradiction leads to another contradiction. As another example, s could be "Theory **T** is self-consistent."

A contradiction in a theory is an especially pernicious problem. I pointed this out [10] by supposing that Tweedledum and Tweedledee were debating the question whether a theory **T** is self-consistent. Suppose that Tweedledum derives from **T** two propositions v and w , which are obviously inconsistent with one

another, and claims that the inconsistency is inherent in the theory. Suppose that Tweedledee, in reply, uses a different method to derive $\sim v$ from T , and claims that Tweedledum is wrong. But, since it is the consistency of T that is in question, he has not refuted Tweedledum's claim: v and $\sim v$ could both be derived from an inconsistent theory. In order to refute the claim, he must show that either v or w *does not follow* from T ; that is not the same as showing that either $\sim v$ or $\sim w$ *follows* from T , if T is self-contradictory. I believe that this was part of the problem in the earlier debate between Dingle and McCrea on whether the special theory entailed asymmetrical ageing. Since both then believed that the theory was self-consistent, each assumed that deriving from the theory a result that contradicted a result that the other had derived was sufficient to refute the other's derivation, whereas they could both have been valid derivations from the inconsistent theory. That is what Dingle meant when he told me that, if the theory were consistent, it would necessary to disprove one of the two inconsistent results to resolve the paradox, and that showed why he eventually came to believe that the theory was self-contradictory.

4. Experimental Support for the Theory?

I also pointed out in my book that a claim of an inconsistency cannot be refuted by experiment. For example, we could derive from my self-contradictory theory T any statement of the form, "If you do experiment X you will get result Y ." That means that T can match any experimental result whatever; for example, it could match all the experimental results that are claimed to support special relativity. This immediately shows that it is not valid to refute a claim of an inconsistency in the special theory by appealing to experimental results, since an inconsistent theory can match the results of all past, present and future experiments.

An interesting assessment of the experimental support for the special theory was made in the following statement by N. Sperling [11]: "The Special Theory of Relativity is 'only' a 'theory', but it has been tested so many times and so critically that it is something like 99.9999999% likely to be right." In order to justify such an estimate of probability, there would have to have been 10 billion successful experiments performed, with at most one unsuccessful result. That number is larger than the number of inhabitants of the earth, so it is highly unlikely that so many experiments have been done.

Sperling also estimated the probabilities of some other possible features of the world (page 18): astrology has a confidence level of 0.00001% (1 part in 10 million) and Santa Claus a confidence level of 0.0001% (1 part in a million). In other words, according to this scientist, astrology is 1000 times more likely to be right than special relativity is to be wrong, and Santa Claus is 10,000 times more likely. That shows real faith in the theory.

Many of the attempts to refute Dingle's criticisms of the special theory appealed to experiment to refute his claim that the theory is inconsistent, and Dingle published some criticisms of these experiments. For example, he argued that many supposed experimental confirmations were not valid because they were based on velocities of particles being inferred from electromagnetic theory rather than by measuring the time taken by a particle to travel a known distance. However, I wish to go further than

that: I wish to say that experimental results are completely irrelevant in attempting to refute a claim of a logical inconsistency in the theory. The presence or absence of an inconsistency in the special theory is a property of the collection of words and symbols that appeared on certain pages of a journal in 1905; it cannot be settled by experiment, any more than the presence or absence of a spelling error in those pages could be settled by experiment.

If there is a logical contradiction in the theory, that makes it immune from refutation by experiment, because, according to the rules of logic, any experimental result that has been obtained can be matched to the theory.

When my book was nearly finished, it was announced that neutrinos had been shown to be traveling faster than light, which supposedly violated the special theory. Herbert Dingle would not have been surprised at the result, since he stated more than once that he believed it was possible that particles might have already travelled faster than light, but that the scientists involved would have estimated their speeds using conventional theory and would therefore not have discovered that. For example, here is what he wrote in 1962 [12]:

"Apart from this theory there is no reason to suppose (among other things) that the velocity of light, c , is unsurpassable, though if a higher velocity were reached in the laboratory the equations of the theory would necessarily register it as lower than c ."

In the experiments that supposedly found neutrinos traveling faster than light, the speeds were found by measuring the time taken for the particles to travel a measured distance. The results were greeted with extreme skepticism, one of the more interesting examples being a professor of physics whom I quoted in my book, who announced that he would eat his boxer shorts on live television if the neutrinos had actually traveled faster than light.

5. Dingle's Argument

In Appendix I of my book I republished a "Proof that Einstein's Special Theory cannot correspond with fact" that Herbert Dingle had originally published in *A Threefold Cord* [13]. His proof depends on a long and closely reasoned verbal argument, which I now attempt to summarize.

In his argument Dingle described a thought experiment that is based entirely on Einstein's description of the procedure for synchronizing two clocks, and does not involve any other part of the special theory. He described how, in a group of bodies relatively at rest in a co-ordinate system, with a clock on one body which is taken to be the origin of the co-ordinate system, the time of any event can be found by letting a beam of light proceed from the event to the clock at the origin and subtracting from the time read by the clock the quantity r/c , where r is the distance from the event to the origin and c is the velocity of light. In other words, it is not necessary to have synchronized clocks at every point in the co-ordinate system to find the time of any event; one clock at the origin is enough.

Dingle then went on to suppose that there were two such groups of bodies, all the bodies in the second group being relatively stationary and moving with uniform velocity with respect to the first group. He envisaged two swarms of stars passing through each other, with the two clocks at the two origins pass-

ing one another at an instant when both clocks read zero. He then supposed that at some later time two stars, one in each group, collided, and considered the times of the two events as given by the readings of the two clocks after making allowance for the time for the light to travel from the event to the clocks.

Dingle supposed that the two clock readings for the second event were different. Then, since the two clocks had agreed when they were together, they must have run at different rates; if one showed twice the reading of the other, it must have run twice as fast. He made the point that the event represented by the collision of the two stars did not belong to one coordinate system rather than the other. From that he argued that, if the times given by the two clocks are different, the difference should not depend on the pair of events chosen. That result is in conflict with the requirement of the Lorentz transformation, which gives a different value for the ratio of the two clock rates depending on the event chosen for the comparison.

That argument of Dingle's, as reproduced in my book, was also reproduced by Phipps in his book *Old Physics for New* [14]. In his review of Phipps's book, Good [15] claimed that Dingle's argument was unsound, and attempted to refute it. He quoted the equation that Dingle had used to show how the Lorentz transformation gives different values for the ratio of the two clock rates, in the following form:

$$\frac{\Delta t'}{\Delta t} = \gamma - \gamma \frac{v}{c^2} \frac{\Delta x}{\Delta t}$$

where v is the relative velocity of the two clocks, c is the velocity of light, and $\gamma = 1/\sqrt{1 - v^2/c^2}$. Good went on to write:

"But actually both sides of the equation are equal to $(1 - v^2/c^2)^{1/2}$ whatever the value of v and whatever events we choose, assuming of course that we are referring to a pair of clocks for which the one has velocity v relative to the other. Dingle seems to be saying that the right side of [the equation] can have various values while the left side remains fixed. I think Dingle made a mistake here."

Unfortunately Good misinterpreted Dingle's argument, because it is obvious from his statement above that he assumed $\Delta x/\Delta t$ to be v , whereas Dingle took Δx and Δt to be the space and time intervals between the two events that he mentioned, namely the meeting of the two clocks and the collision of the two stars, and that is a completely different assumption. So Good failed to refute Dingle's argument and, as far as I am aware, that argument of Dingle's has never been refuted.

6. Conclusion

In the forty-year period since the publication of *Science at the Crossroads*, the scientific community has celebrated with great

pomp and ceremony the centenary of Einstein's birth in 1979 and the centenary of his "*annus mirabilis*" in 2005. Unfortunately, however, the scientific community has shown no inclination to include a critical re-assessment of his theory in any such celebrations. As a result, the deification of Einstein has become more ardent than ever, and critics of the special theory continue to be dismissed as crackpots. Scientists continue to ignore the self-evident contradictions in the arguments that were used by Herbert Dingle's critics to answer his criticisms, and editors of mainstream journals refuse even to consider papers critical of Einstein's work. In their avoidance of criticism of relativity, scientists are weakening science instead of strengthening it, for criticism ought to be a vital part of science.

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